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ABSTRACT

This paper attempts to trace a line of theory development in technical communication from Dwight Stevenson's 1977 call for "instrumental effectiveness" in theoretical inquiries, based upon careful inductive research methods, to later examples of more speculative and inadequately documented theories about what constitutes the field's intellectual domain. While acknowledging the importance of interdisciplinary inquiry, the paper critiques some inquiry approaches for: (1) an excessive deviation from necessary traditional constraints on the field's legitimate inquiry domain, and for (2) an excessive reliance on composition studies theory for specific intellectual guidance. The paper argues for a restoration of more field-appropriate theory-building as articulated by Stevenson, Anderson et al.; one based upon the constraints of inductive evidence and instrumental effectiveness. Twenty-seven notes are included. (Author/PRA)

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Theory Building in Technical Communications: Instrumental Effectiveness versus Speculative Questing

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ABSTRACT

This paper attempts to trace a line of theory development in Technical Communication from Dwight Stevenson's 1977 call for "instrumental effectiveness" in our theoretical inquiries, based upon careful inductive research methods, to later examples of more speculative and inadequately documented theories about what constitutes the field's intellectual domain. While acknowledging the importance of interdisciplinary inquiry, this paper critiques some inquiry approaches for (1) an excessive deviation from necessary traditional constraints on the field's legitimate inquiry domain, and for (2) an excessive reliance on Composition Studies theory for our own specific intellectual guidance. This paper argues for a restoration of more field-appropriate theory-building as articulated by Stevenson, Anderson et al.; one based upon the constraints of inductive evidence and instrumental effectiveness.

In Tom Wolfe's acerbic little book on the condition of American architecture, From Bauhaus to Our House (1981), the Bauhaus tradition is excoriated for subverting a native spirit and impulse in architecture and blamed for what Wolfe considers the sad plight of design and construction in modern America. He attributes blame to the misguided efforts of a small but influential group of early twentieth-century European theorists

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architects like Walter Gropius and Le Corbusier and Mies van der Rohe, who engaged in "the sheer intellectual competition of the theories" but who, themselves, "did little or no building." The theories, Wolfe says, "began succeeding one another in a berserk rush" so that, eventually, "for the ambitious architect, having a theory became as vital and natural as having a telephone." Wolfe argues that as modern architecture evolved through more complex and arcane discussion, it moved farther and farther away from its essential purpose: namely, to please the user; and its gradual justification became its own ability to conform with predetermined theory.[1, p.89, 121] But Wolfe's derogation of a misapplied theorizing is just one of many such critiques, best expressed, perhaps, in Goethe's famous lines from Faust, "Grau, theurer Freund, ist all Theorie/ Und grun des Lebens goldner Baum" ("Gray is all Theory; green grows the golden Tree of Life") and, more recently expressed in Stephen J. Gould's remark that

> We all have a tendency to spin universal theories from a limited domain of surrounding circumstance. Many geneticists read the entire world of evolution in the confines of a laboratory bottle filled with fruit flies.
> [2, p.16]

Theory Building: Fashionable or Responsible Activity

Whether one agrees fully or not with Wolfe's views, perhaps there are some useful parallels to our own situation in Technical Communication worth noting; namely, that in the practical arts, theory building carries particular responsibilities; that any rush to theorize is, perhaps, symptomatic of some degree of field instability; that, when it becomes a more fashionable than responsible action, theory-building can dilute its own serious purpose and begin to trivialize the importance of field inquiry.

Bruffee writes about "the tendency to classify our knowledge into 'theory' and 'practice' as premised on the hierarchical notions of cognitive knowledge, whereby theory, or concept making as the product of the mind's 'inner eye, represents a more powerful level of thought, of which practical application is the less privileged and less powerful. Using a phrase from Stanley Fish, "theory hope," he defines it as "the belief that whatever a theory sanctions us to do is surely correct, whatever we learn under its aegis surely true, and whatever results we get using its methods are surely valid." [3,p.781-782] This nicely describes, I think, how some think of theory in our own field and helps explain something of the fascination for ennunciating and devising new "theories." Anderson, describing the situation in 1980, might not appreciably change his opinion ten years later: that theoretical research is generally weak and "actual investigations designed to develop a theoretical understanding of technical communication or to test some theory about it are **extremely** rare;" that "the teaching discipline is failing to identify problems that need solving" and that they love "to debate, but not to investigate." [4, p.275] Commenting in a 1985 editorial, the editor of **Technical Communication** also lamented the lack of a research tradition and remarked that "Typically, we work on the basis of intuition and folklore" and that "We need to be able to say that experimental research has proven conclusively that our recommended approach is superior." To accomplish this, he further argues that it is essential we develop a cadre of dedicated researchers who 'feed on and advance from each others work' as that work is systematically advanced in the form of good research journals and via "a cohesive body of research literature." [5, p.4-5]

Technical Communication and Composition Studies: Similarities and Differences

There is a similarity between the kind of charges North makes against Composition Studies generally and those that might also be made against Technical Communication. For example, he suggests that the central business of an experimental community [which ours should primarily be] should be **certification** and not **discovery** [6, p.156]. And, in quoting Braddock, North concurs in a critical assessment of Composition Studies that, as we have just seen, seems to fit our situation as well:

> Not enough investigators are really informing themselves about the procedures and results of previous research before embarking on their own. Too few of them conduct pilot experiments and validate their measuring instruments before undertaking an investigation. Too many seem to be bent more on obtaining an advanced degree or another publication than on making a genuine contribution to knowledge, and a fair measure of the blame goes to the faculty advisor or journal editor who permits or publishes such irresponsible work. And far too few of those who have conducted an initial piece of research follow it with further exploration or replicate the investigation of others. [p.156]

While there have been some claimants for philosophical and theoretical leadership within the field, no one to my knowledge has yet earned the mantle. Again, the situation is similar to Composition Studies where, as North says, "the philosophical community has not yet had an [articulate] apologist.'[p.111] There seems general agreement about a fundamental weakness in the areas of theory and philosophy, as well as in the experimental, and the best work is simply not coming from academic quarters; we still take a back chair to the more stringently tested and practical work coming from the practicing professions. Again, the conclusions that North draws from his discussion about the philosophers of

Composition are remarkably apropos, I think, to the field of Technical Communication:

> The danger of Philosophical inquiry is clear: To move outside the bounds of method, whatever the short-term gains in reform - especially when. . . the move finally threatens to subvert the power of a dialectic altogether - is to risk the demise of methodological integrity. . . . . the impulse that drives such Scholarly reformism, the urge to make the world a better place, is surely understandable. But modes of inquiry are in some ways very restricting things; to accept what power they offer is to accept, as well, that there are things they cannot do - or at least, that they cannot do and still be themselves, sustained by the investment of the community of inquirers. [p. 115]

Technical Communication, though different in significant ways from Composition Studies, nevertheless shares some similar problems. Like Composition Studies, we still have a tenuous definition of ourselves, a confusing marketplace of ideas, and a sense, sometimes, that things seem to "be spinning centrifugally to pieces." As Robert Connors observes in a recent perspective piece:

> The field of composition studies faces many difficult questions today, and assigning them priority is difficult. They range from obvious areas of investigation like 'What cognitive domains are important to writing?' and 'What makes written discourse effective?' to more abstract questions of the philosophical place of language and the interactions of symbol systems. In the past we have tended to give priority to the raw questions of how better teaching could be accomplished and to distrust anything smacking of non-pedagogical interest, but over the past decade, series of inquiries about the intersection of discourse, meaning, ideology, and philosophy have arisen, and **these questions threaten to break the field open.** The issues include these: Are we indeed a field? If so, what sort of community of discourse are we? What kinds of knowledge do we share? What sorts of problems do we communally believe important? What sorts are soluble and insoluble? Where do we find knowledge? How do we share it? What are our criteria for believing something true? How do we situate our daily work in the universe of morality and usefulness? [7, p.231; my italics]

In his defense of the special value of a historical approach, Connors notes that, in "pursuing research paths so disparate. . . many thoughtful people have feared that the discipline may fly apart like a dollar watch," that "part of the intellectual task of composition studies today is to understand and unify itself as a discipline" [7, p.235]. In a similar vein, Steven North, in The Making of Knowledge in Composition (1987) also remarks that:

> It might not be too much to claim, in fact, that for all the rhetoric about unity in pursuit of one or another goal, Composition as a knowledge-making society is gradually pulling itself apart. Not branching out or expanding, although these might be politically more palatable descriptions, but fragmenting: gathering into communities or clusters of communities among which relations are becoming increasingly tenuous. [6, p.364]

Conceding that Composition "can safely be termed a field," North thinks that "to consider [it] a discipline is to ask a good deal more" [p.364]. Such comments closely resemble similar remarks by Carolyn Miller at the 1985 CPTSC meeting, to the effect that "technical communication is not a discipline [but] a set of related activities and phenomena that provides a locus for the methods and insights of many disciplines."[8]

While there seems little likelihood that Technical Communication will fly apart like a dollar watch, there is growing concern that some of our theoretical inquiry tends to pursue tangential issues that are either non-productive or distracting from more important matters we should be discussing as a profession. It will, in fact, be the main contention of this paper that (1) some of our current theorizing is speculative beyond what available evidence would allow, that (2) technical communication has, by virtue of its own history and function, less opportunity for the kind of

theory-building that might be more appropriate in other disciplines, that (3) the profession needs to exercise greater responsibility and be more cautious and circumspect in its theorizing tendencies; and, finally, (4) that whatever theory-building is done should emerge only after more rigorously applied inductive methodologies can provide clearer permission for the testing of reasoned claims or conclusions. To the extent that one can agree about certain problems facing the profession, we might suggest four possible solutions: (1) that we develop a more complete sense of our own unique history; (2) that there be a more concerted effort to achieve wider consensus on the nature of the particular reality that technical communication addresses - and what written knowledge does in technical communication; that (3) there be a more intellectually challenging, and dialectically motivated, exchange within the field; and, finally, (4) that empirical efforts be constantly improved and expanded. Our primary intellectual obligation should be to elucidate the **disciplinary nature** of our own profession - insofar as that can be defined - within the more general field of composition studies by methodologies that build upon valued intellectual precedents rather than by pursuing currently fashionable speculations.

Multidisciplinarity and the Field-Discipline Debate

One of the ironies of both our fields is that, at the same time we wish to be more multi-disciplinary, we reveal certain difficulties in defining what, precisely, we are. If we are, as Miller claims, "a set of related activities" rather than a well-defined discipline, then certain obvious problems automatically follow for any theory building. First, since boundaries tend to become more amorphous as they become more over-lapping,

there would seem no effective way of gauging what might (or might not) be useful speculation in the way that some of the more defined sciences are able to. What constitutes a "related" activity? or a "set of related activities"? And does there come a point when the field of Technical Communication becomes so diffuse, through the accretion of "related activities" that the possibilities for a broad intellectual consensus becomes, increasingly, more difficult, or even impossible? It seems plausible that the more inter-disciplinary the field becomes, the more difficult any guiding theoretical assumptions will be. Also, the more inter-disciplinary the field is, the more "theory-prone" that field will become, and the more theory laden it will be. There seems some inevitability to this seemingly circular process, which should contribute to a diffuseness and the growing difficulties of achieving a singular disciplinary focus and clarity. Extending the logic of this one additional step: the more theorizing that is done, the more unlikely it becomes that any general conceptualization will suffice and the more amorphous become its expanding boundaries. On this point, North has agreed, that while sometimes inquiry has been quite thorough, it has, just as frequently been "painfully careless;" that "the community's interests have consistently run wider than they have deep; there has been a good deal more dabbling than dialectic."[6, p.96] One antidote to this problem is, perhaps, a greater need within the profession for more self-criticism (through dialectic) of this tendency to seek its own expansion into new domains. The danger in not practicing a greater restraint is quite possibly the risk of becoming, vis-a-vis other professions, trivialized. There are, of course, some professions that, indeed, have suffered the intellectual scorn of

colleagues by pretending to claim more than their research can capably demonstrate. Of more immediate concern to technical communication is the danger that a deficient theoretical conceptualization of proper domains will likely result in the inadequate and widely divergent training of graduates in the field.

Theory Building: From the Instrumental to the Speculative

The following discussion attempts to trace a pattern of theorizing that has evolved during the past decade. For the purposes of illustrating the point, selected essays have been chosen from scholars who represent divergent viewpoints and whose work here represents something of the trends and developments I wish to critique. While these essays are not all causally related, it can be argued that they are representative of lines of argumentation that suggest conflicting perspectives about the purpose, function and limits of theory-building. Each discussion relates to, and even builds on the other, such that each seems to provide an implicit permission for further enlargement of the discussion. That this permission is sometimes misapplied or misunderstood, and leads in directions that are not always - from this point of view - constructive to the field, is also discussed. And, while these discussions are not always dialectically related like, for example, the lively exchange that occurred some years ago between Janice Lauer and Ann Berthoff (or, more recently, between David Dobrin, Caroline Miller, Ann Berthoff, et al.) they still might be read in a more loosely dialectic sense as "building" upon the edifices of each other.[9]

I should add that to single out each author here for some measure of criticism is clearly to honor and acknowledge their influence on the prevailing rhetoric and direction of the field. One result of this exercise is, hopefully, to stimulate the kind of dialectic among scholars seen elsewhere but which, in Technical Communication, is largely still missing and needed. Bertoff and North, among many, have encouraged more philosophizing and debate; North, for example, encourages "efforts to orchestrate certain lines of argument in the name of sustaining the dialectic." [6, p.112]

Stevenson and the Empirical-Research model

Dwight Stevenson's 1977 article, "Towards A Rhetoric Of Scientific and Technical Discourse," [10] stands as a touchstone for my central thesis and begins with a complaint that the field has done little to keep pace "with the changing and developing needs in scientific and technical disciplines." The problem, he suggests, is due to both the disdainful attitudes of rhetoricians towards technical writing as well as a limited vision of what rhetoric can contribute to technical and scientific discourse. A clear solution would be in the form of a rhetorical theory that addresses more directly technological interests. Stevenson mentions, for example, the rapid proliferation of environmental impact statements, and the need for developing good models and designs that facilitate more maximally usable documents. He observes the research possibilities in the rhetoric of visuals, as well as other forms of task-oriented discourse. Yet he laments the fact that there "seems to be virtually no rhetoric to go on" and, in the case of manuals, he observes that "we have not been able to generalize, successfully, principles by which effective manuals are designed:"

> As a consequence. . . the only workable way to tell whether a manual is any good is to try it out, to have someone actually follow the instructions while someone else watches to determine empirically where the trouble spots are. While this procedure certainly works, it is neither efficient nor is it directly helpful to writers of subsequent manuals. It still leaves unanswered, except in an empirical sense, the basic question, "How do you know an effective manual when you see one?' To me as a teacher, and more importantly to scientific and technical people who must write and evaluate manuals, this unanswered question is frustrating. [10, p.8]

Most important to Stevenson's charge, however, is his clearly articulated formula for research practices that lead to theoretically sound principles upon which effective practice can be based:

> [Theory] must be developed inductively by observers or researchers who go into the places where both effective and ineffective scientific and technical communication is being carried out. It cannot be developed by scholars who are content merely to remain in their offices, to work deductively, **and to theorize about how existing rhetorics might be extended to account for changed communication situations. The measure of effective scientific and technical communication must be of its instrumental effectiveness, not of its comprehensibility in terms of traditional rhetorical theory.** [10, p.9, my italics]

Implicit in this central touchstone statement are important notations: (1) that there are important differences between what theory means for technical communication ("its instrumental effectiveness") as well as what it means for other disciplines, and (2) that whatever theory does emerge must be based upon an empirically adequate body of accumulated data. Stevenson's call for a new rhetoric of scientific and technical discourse is, in a sense, a moot issue as Elizabeth Harris shows in a subsequent essay - that "an inductive rhetoric of scientific and technical communication will also be comprehensible in terms of an inductive theory

of discourse that takes traditional rhetorics into account." [11, p.625] More specifically, Harris suggests that Kinneavy's model should adequately meet Stevenson's needs.

Speculative Permission and the Abandonment of Instrumental Effectiveness

In a subsequent critique of Stevenson's views that, I believe, mark an important departure from his clearly restrictive injunctions on theory and practice, Ben and Marthalee Barton suggest that Stevenson's views are "too instrumentally oriented, too prescriptive, too aimed at providing a basis for pedagogy." Their view is that Stevenson has "unduly inhibited **the more speculative inquiry that is . . . characteristic of literary theory**" [12, p.132; my italics]. The Bartons argue for a broader perspective--and a broader warrant--for technical discourse, where the methodology emphasizes **shared** features rather than the more reductionist approach of **distinguishing** features. Curiously, they regard Stevenson's approach as contributing to notions of both "alienation" and "binarism" in his so-called insistence on technical writing as "a set of specialized skills practiced by a trained elite" who become increasingly divorced from mainstream society via the "'two-cultures syndrome.'" They argue instead for an inquiry method that "ultimately. . . must establish the theoretical basis for a new truth-claim for technical discourse." [p.138] Through a post-positivist theory of discourse, distinctions would become less relevant and "the binary opposition of the demonstrable truth of technical discourse and the fiction of literature is thus collapsed. Both technical and literary discourse are symbolic reconstructions of reality--ultimately, fictions" [p.134].

While the Bartons' are critical of Stevenson's "prescriptive use of instrumentally oriented theory (I would argue with their pejorative

implications of the phrase) Stevenson is clearly unapologetic in his argument for an instrumentally oriented theoretical foundation for anything proposed or undertaken in technical communication. In fact, when one reviews Anderson's prescriptions for what constitutes effective theoretical work in the field, it clearly reflects a theorizing that also has a similar instrumental purpose and seems perfectly consistent with Stevenson's earlier stated views.

> The third technical communication discipline is the theoretical. Instead of trying to find out **what works best** - on the job or in the classroom - it tries to understand **how** things work. It asks such questions as, 'What is a reasonable way of describing what happens when technical information is being communicated?' and 'What is a reasonable way of explaining why one communication seems to work better than another?' When trying to select from alternative answers to such questions, the theoretical discipline applies criteria that ask, 'Which most satisfactorily explains the phenomena under consideration, at least insofar as we presently understand these phenomena?' [4, p. 274]

The Bartons' article is important in this evolutionary process, I think, because it gives permission for a new line of inquiry that essentially extends the realms of the speculative ("shared features") and collapses general discourse theory with technical communications theory on the basis that each are symbolic and, therefore, fictive reconstructions of reality.

One example of this new license can be found in the writings of Marilyn Schauer Samuels. In her 1985 article, "Technical Writing and the Recreation of Reality" the distinctions Stevenson was trying to make in his 1977 article are virtually eliminated. Samuels begins by stating her thesis:

> I propose that to discover a theoretical basis for the discipline of technical Writing we need to put aside these familiar distinctions and try out new ones: there is no such thing as creative versus non-creative writing. All writers are creative; and everything written is a creation or recreation of reality. Accepting these premises allows us to develop a rhetoric that categorizes each kind of writing as a separate creative act. In this context, technical writing is one special kind of creativity, different from other kinds, **but neither more nor less accurate in its depiction of reality than a poem or play.**
> [13, p.3 my italics]

Samuels is clearly not interested in the same "world" or professional issues that Stevenson is; where he seeks after instrumental effectiveness through rigorous inductive methods of investigation, she seeks after what he precisely enjoined against: comprehensibility through a more deductive and speculative inquiry.

In a later discussion, "Is Technical Communication 'Literature'? Current Writing Scholarship and Vico's Cycles of Knowledge," (1987) Samuels attempts to underscore her ideological beliefs via support from historical analogues. In defending a social view of discourse, however, she rejects any possibility of a discourse taxonomy; according to Samuels, the idea of a **discourse continuum** befits the 'paradigm shift' from the age of analysis (Kinneavy) to the age of synthesis (Larson). She describes a rather idiosyncratic history, where "[Merrill] Whitburn's approach places him in the tradition of modern literary critics such as Matthew Arnold and T.S. Elliott" [sic].

> Unlike Elliott [sic], however, who views the interaction of thought and feeling in literature as an opportunity forever lost, Whitburn sees dissociation [of sensibility] as a temporary phenomenon in the historical cycle. In fact the time is right for applying a rhetorical antidote.
> [14, p.51]

Though Samuels writings have been praised as "representative of the more forward looking scholarship now being conducted" and challenging "views considered traditional within segments of the technical writing community" [15, p.1] they reflect, in this view, a significant departure from technical communications traditionally narrower but more clearly focused purposes and functions. Samuels' speculations reflect and incorporate, at virtually every point, current debate in Composition Studies and seeks to minimize most distinctions between it and Technical Communication. Her analysis, indeed, seeks "comprehensibility" at the expense of "instrumental effectiveness" and represents the antithesis of Stevenson's injunctions. The expectation - that her audience will uncritically accept her overlapping truth claims about the viability of historical cycles, paradigm shifts, rebirths of synthesis, etc. is, I believe, both simplistic and finally naive. To make assertions like "there is a cyclical pattern of man's pursuit of knowledge," that "the history of the western world exhibits repetitive patterns of intermingling, separating, re-allying and separating again between the humanities and the sciences" which are "reflected in current rhetorical theory" [12b, p.48-49] seem slightly disingenuous and sophistic. To say that technical writing "is a creation or recreation of reality" and "is different from other kinds, but neither more nor less accurate in its depiction of reality than a poem or play" [13, p.3] both begs the issue and overstates the differences. On the contrary, the instrumental distinctions that Bazerman makes about textual recreation in his Shaping Written Knowledge (1988) are more carefully crafted and stated. His analysis of three texts, from molecular biology, sociology, and literary criticism, demonstrates that "the three statements

of knowledge are three different things" and that "the phenomena are not equally fixed prior to the essays." [16, p.46] One might argue, in short, that Bazerman's purpose is to demonstrate the distinguishing features, rather than the shared features, of the experimental article in science from other rhetorics: "In mediating reality, literature, audience, and self, **each text seems to be making a different kind of move in a different kind of game.**" [16, p.46, my italics]

Samuels writes approvingly, for example, of an ecological approach to writing, as advocated by Marilyn Cooper in "The Ecology of Writing." But does this mean that Samuels, like Cooper, "advocates the rejection of the 'cognitive process model' because it erroneously conceives of the writer as a 'solitary author' who works alone within the privacy of her own mind"? It seems clear to me that Cooper's "ecological" model is simply inappropriate for our purposes and objectives in technical communication. Cooper is talking about certain kinds of exploratory writing when she writes that "a further value of the ecological model is that it can be used to diagnose and analyze such situations, and it encourages us to direct our corrective energies away from the characteristics of the individual writer and towards imbalances in social systems that prevent good writing." [17, p.373]

Ecological approaches are broader and more comprehensive then contextual, which in turn are more comprehensive than cognitive. Such expanding networks of theoretical enlargements seem to me reminiscent of the photographer's paradox in the Antonioni film, Blow Up; he keeps enlarging the tell-tale photographs of a murder scene until the dots which constitute the photograph become so increasingly distanced from each other

that the closer he thinks he is getting to the truth of the fact, the more distorted and uncertain the event seems to become for him.

Technical Communication and Social Analysis: Theory Building in Violation of the Stevenson Principle

In yet another proposed methodology, also based upon a speculation that appears to exceed available evidence, Carol Lipson suggests a further permission of the speculative license that both the Bartons and Samuels have encouraged. In Lipson's recent article, "A Social View of Technical Writing" (1988), she argues for a pedagogy that incorporates active interventions based upon a knowledge of discourse communities and cultures. Since language "does not just communicate information but also creates a social reality and reproduces a system of shared values and meaning," therefore, organizations are not just 'discourse communities' but discreet cultures, "each having its own values, myths, traditions, rituals and common procedures." [18, p.7-8] In Lipson's discussion of professional social groups, she assumes that a careful examination of their language behaviors will reveal elements of their particular socializing tendencies "as well as [the] values and doctrinal principles of the group." Depending on whether we study lawyers, doctors, engineers, physicists, clergy (and depending on the established degree of socialization implicit in the profession), it is possible to construct highly specific interventions discreetly tailored to that particular discipline. Through a careful analysis of minimal linguistic cues, a technical communicator is provided a maximum range of praxis in the social and cultural life of a corporation. By extracting from common usage certain salient features of the group,

communicators can give highly specific interpretative meanings and, from this, draw useful profiles of the particular business or corporation. Through such action comes "empowerment" and the ability, as it were, to speak the 'language' of that particular profession.

Lipson seems to be arguing for the incorporation into the knowledge base of technical communication a new sub-discipline, discourse socialization.

> I wish to consider specialized technical fields in similar social or cultural terms. I believe such social analyses can fruitfully inform our understanding of specialized technical discourse. . . . I would extend Kuhn's claim to suggest that not just study of the research but also study of the language use of the specialized communities **will benefit from attention to the social characteristics of the specialized knowledge communities.** My thesis is that even in occupational use for technical fields, the language is intimately related to the culture of the community.
> [18, p.8 my italics]

Since each field has "its own highly distinctive type[s] of discourse, with specialized vocabularies and particularized handlings of form," the technical communicator should be empowered to the extent that he/she "is intimately related to the culture of the [discourse] community" [p.8]. In other words, the theoretician/researcher's task is to chart the myths, traditions, rituals, and procedures of any given field or discipline--via interdisciplinary knowledge--as these are manifested in language and, thereby, provide comprehensive and effective interventions, both at the meta-professional levels and in more discreet terms of specific companies or corporations.

> Fields such as science, law, engineering, medicine, and nursing differ not just in subject and function, but also as social and political entities. Their approach to writing will differ

> accordingly. Analysis of the social and political characteristics proves a powerful factor in explaining the way language is used in the writing of members [p.10].

The logic of Lipson's claim seems quite clear: the only way of getting at the language use of any discipline is through a knowledge of its social and cultural characteristics--thus effectively transforming technical communicators primary purpose and function from language expertise to socio-cultural expertise.

Unfortunately, Lipson's evidence is rather thin gruel. She argues, for example, that the use of nominalizations and noun-strings provide significant insight into the value-system ("the thingness of things, the factuality of facts, is enforced by the consecutive recital of nominals. . ."). Using "cell alteration" rather than "altering cells," she suggests, tells us much about the value system of science. But--one asks--how much can we legitimately infer from such instances? and what legitimate generalizations can we draw from such bits of data? Though one can agree, in commonsense terms, with some of what Lipson suggests, nevertheless, there remains quite insufficient data upon which to build the kind of theoretical foundation that she argues for. Typical of other similar writings, she argues for considerably more than her evidence permits. Notice, for example, her claims for legal writing--what she calls a "strong" profession:

> Legal writing is highly formalized and even formulaic both in style and structure. There is very little deviation from standard formulations in texts lawyers produce. Lawyers are taught everything they write will come under immediate or eventual scrutiny by other legal experts. Thus, collegial scrutiny is a strong force in this field. The writing conventions in this field affirm and reiterate the values and methods intrinsic to law: the valuation of uniformity,

> past precedent, and adversarial, ritualized argumentation. The writing conventions that affirm these values serve to reinforce the socialization stage as the practicing lawyer applies them [p.15].

But what is it about legal language itself that differentiates it from the language of all other disciplines and reveals its own unique community and culture? How does the culture (if that can be sufficiently adumbrated) inform the legal professions own discreet use of language? Nothing Lipson says here about legal discourse cannot be said equally of many other discourses as well. Like Stevenson, Lipson begins her discussion by claiming that most recent treatments of technical communication ignores a fundamental point: he, an instrumental rhetoric; she, a social discourse theory. Comparatively, they measure not only the distance between professional concerns over the period of a decade but, also, the fundamental distinctions between what constitutes instrumental theorizing at one moment and speculative theorizing at another. In effect, Lipson is ultimately less interested in HOW information gets communicated than in WHAT social and cultural values are revealed through language. Accordingly, organizations are more interesting to the extent that they can be studied as cultures, and the challenge for researchers is to build the most detailed descriptors of as many discreet discourse communities as possible. In an interesting reversal of our expertise, we come to dealing with the information through sociological and anthropological explanations. Engineering, we are informed, is a socially "weak" field because there is minimal time for socialization as in "strong" professions like law or medicine. Engineering, she suggests, "lacks the one characteristic traditionally deemed the essence of professionalism--a community of shared values." Thus, "one might predict, then, that engineers would show far

greater willingness to individualize their writings for their audiences, for particular contingencies, than have other professionals, such as scientists and lawyers." [p.17] However, there is here (as Bazerman would argue) no 'ground established between the claim and the ground upon which the phenomenon rests.' This statement of knowledge is entirely dependent upon the good will of the reading audience; that is, "a sympathetic audience is more likely to expend the effort to reconstruct from partial indicators the meaning most congruent to the argument--a process that may be called reading in the intended spirit." [16, p.34,37] Thus, such a claim is maximally dependent upon the plausibility of the argument and the reader's sensibility to relive imaginatively its thesis; in this sense, Lipson's claims seem more proximate to literary analysis than to either sociological or scientific analysis. [16, pp.19-55]

Lipson provides an interesting and instructive ironic twist to the 'Stevenson principle' when she writes that "we as yet have only partial explanations" to a full understanding of, for example, the social constructs of engineering writing; that "descriptive work is still needed before one can fully apply the theoretical model I have presented with respect to the use of language in engineering" [18, p.18]. Therein, however, is part of the problem: that the theoretical model precedes an adequate evidentiary basis for its formulation. Lipson's claim is both insufficiently supported by inductive reasoning and data and therefore fails Stevenson's and Anderson's "test" for validity. In effect it remains a problem outside the realm of solvability and, therefore, not a worthy problem-solving assignment for the profession. This, however, does not mean that it cannot, at some point, become a 'problem worth solving.' In

order to become so, though, it necessitates a stronger warrant than is provided in the argument. Lipson does not yet have a theory to make, though that does not preclude her having one in the future and one deserving of marshaling the investigative forces of the profession in the laborious but necessary task of substantiation. Furthermore, the theoretical model itself needs a clearer definition and a better notion of how it might be applied (Lipson has promised a fuller description in a future publication that will "analyze the use of language in engineering, in the context of this theoretical framework").

Technical Communication and the Pragmatics of Theory-Building

In my view, the kind of theorizing that still works best is the pragmatic approach prescribed by Stevenson; it has a pedigree that historically connects it with an inductive tradition which has produced solid results in the past. By using Connor's historical methodology as a fulcrum for making assessments, we can more effectively distinguish between non-productive and productive theorizing in terms of results achieved. Connors' account of the breakthrough text of Mills and Walters Technical Writing (1954), "arguably the single most important postwar technical writing text," [19, p.342-343] demonstrates the instrumental effectiveness of a theory upon the practice of instruction, such that it "was to revivify what had been in danger of becoming a sterile and mechanical [profession]." [p.343] In their attempts to find a "logical bridge between the professional writing of scientists and engineers and suitable content for students of technical writing," they conducted three investigations and collected from over 300 industrial and research organizations examples of

reports, articles, etc.; next they worked out a theory of what technical writing is, and, third, they studied the content and organization of college courses being actually taught. From this data, they then proceeded to construct their own text, based upon two assumptions: (1) that a rhetorical approach was preferable to the types approach then common, and (2) that the only good criterion was 'does it work'? and that pre-eminence be given to writer-reader relationships. Their investigative process was, in the best sense, inductive as well as focused on the practically useful. And their emphasis on audience became one of the professions most endurable achievements. Therefore, before we abandon these more traditional notions of audience, and attempt to radically redefine our notions of audience-reader-text in the directions of more amorphous notions of communities and cultures, we need to be reminded of the precedent-setting work that has gone before and practice a kind of rhetorical conservatism.[20] As a practical matter, we need to be more observant of our own precedents and traditions and we need to more cautiously define for our own specialized interests the practical relevancy of theories that spill over to us from Composition Studies. Should we fail in this enterprise we risk dissipating the special area of strength: our sense of purpose and sharp focus.

Unique Conditions and Special Constraints

In our profession there is less margin between theory and practice unlike in composition studies which has a greater license to "play" with ideas than does this profession. There are for both theory and practice powerful professional constraints that are, necessarily, restrictive and limiting. Thus, as Pinelli has observed, the scholarship cannot advance

separate from the practice and the first must proceed in concert with the needs of the latter.[21] This is obviously true because there are important issues of credibility involved since our constituency is more precisely defined than is true of composition studies practitioners.

There will then always be a necessary tension between the encouragement to be speculative (Carson) and the injunction to be careful (Anderson). Historical knowledge, as it makes us more aware of traditions and precedents, provides us with the means to anchor whatever speculations we make within a historical continuum. We need to incorporate what is best of current rhetorical and compositional research into our existing methodologies, while avoiding the dangers of becoming clones of literary or composition theorists. Availing ourselves of the best inductive work done by compositionists, like Flower-Hayes protocol analysis, and applying their principles to document design research, for example, and schematic analysis would insure the continuation and improvement of the best kind of inductive research towards praxis.

The Need for Better History and Dialectic

Like Connors, I would also argue for a more rigorous historical awareness in the field. As he suggests, history provides us with the necessary memory to avoid what he calls a "narrow presentism." While history is not always a clear guide, without it we are reduced and diminished in our efforts to build fresh theories. "We become like the agnosiac in Oliver Sacks' case history whose long-term memory lasted only fifteen minutes, and who had to continually re-invent his present because his only past reality constantly disappeared." [7, p.231]

Perhaps more importantly for professions that are definitionally problematic, there is a need for what Connors calls "shared stories." It is through such stories, he suggests, that some unity becomes possible in the confusing "market place of ideas." Historical awareness is also useful to the extent that (again Connors' term) it "interanimates" each other's work, both currently and across generations. Also, and to the extent that one rejects the historicism of relativism and incommensurability, it helps us identify the epistemological ground rules by which we can play the game. Finally, historical knowledge is important, as Smith, Pinelli and others have consistently maintained,[5] in that it helps inform present practice as well as facilitate progress incrementally.

Besides the antidote of an historical awareness, one other antitode to the problems we have been examining here is the development of a more effective dialectic and dialogic - to which, hopefully, this critique contributes. North is, again, apropos of the mark, I think, when he argues that "the shape of Philosophical argument becomes much clearer when it does assume a more clearly dialectical form, the back and forth of argument and counter-argument." [p. 106] While we have become too fascinated with the model of the paradigm shift as an explanation of the obviously important changes that have taken place in Composition over the past twenty years, perhaps a more realistic--if not less satisfying explanation--can be found in the value of the dialectical processes. Blum, in his response to Maxine Hairston's 'Winds of Change' article, suggests an alternative explanation:

> Suppose that progress . . . takes not the form of revolutionary paradigm shifts between dichotomous alternatives, but rather the form of complex, ongoing Hegalian dialectics in which manifold

> theses and antitheses contained within mutually inclusive systems of thought continuously seek resolution. If such is the case, then Hairston's reductive view distorts the multi-dimensional activity it claims to clarify, and thereby impedes, rather than spurs, understanding and progress [22, pp.490-491].

Furthermore, unless theory-making is meaningful and tested through dialectic, a discipline acquires, as Kinneavy says, "no definite concept of what the basic foundations. . .are." "No discipline," he writes, "can be said to be mature if it has not analyzed its own fundamental concepts."[23, p.129] Perhaps one useful analogue to this point from aerospace engineering is made by the late physicist, Richard Feynman, whose criticism of the Challenger disaster was based on the fact that it was improperly designed - from the top down rather than, as was usual practice for aircraft, bottom up. With bottom-up development the quality control and costs are much better managed:

> As deficiencies and design errors are noted they are corrected and verified with further testing. Since one tests only parts at a time, those tests and modifications are not overly expensive. Finally, one works up to the final design of the entire engine, to the necessary specifications. There is a good chance, by this time, that the engine will generally succeed, or that any failures are easily isolated and analyzed because the failure modes, limitations of materials, et. cetera, are so well understood. There is a good chance that the modifications to get around final difficulties in the engine are not very hard to make, for most of the serious difficulties in the engine have already been discovered and dealt with in the earlier, less expensive stages of the process [24, p.226].

Finding a Middle Position Between the Cognitive and the Social

One of the most difficult issues to resolve in technical communication revolves around the authenticity of the text. To what extent does any

text exist independent of its audience? The debate is moot and probably won't be settled soon. But this is not an issue that should involve us directly. Though we might wish to take "cutting edge" positions, our professional posture--as in most theoretical and intellectual positions--should remain centrist and moderate. Confusion and lack of consensus on epistemological and cognitive issues are, I think, more difficult and consequential for us than for other discourse communities. This is so because our "constituents" are less inclined to indulge the academic speculations about language and its relation to meaning in the more strictly academic settings of Composition Studies. Consequently, the longer consensus is delayed or postponed, the more likely the respectful relationship between academicians and practitioners will deteriorate and fracture. E.D. Hirsch's comments about theory in relation to practice in this context are particularly relevant:

> The theoretical aim of a genuine discipline, scientific or humanistic, is the attainment of truth, and its practical aim is agreement that truth has probably been achieved. Thus the practical goal of every genuine discipline is consensus - **the winning of firmly grounded agreement that one set of conclusions is more probable than others** - and this is precisely the goal. [25, p.viii-ix]

One of the more disturbing aspects of recent discourse theory application in technical communication is the seeming eagerness with which we abandon the text for a preoccupation with reader responses and, successively, communal and cultural responses. One senses that the theoretical discussions have not only jumped significantly ahead of confirmable evidence, but have also competed in the efforts to cast the widest possible nets. The effect has been to refocus our attention

increasingly away from any text at all so that critical discussion seems, increasingly, directed towards some kind of deconstructionism, where textual indeterminancy becomes the final goal of some theoretical discussions.

To the extent that we continue to argue over the unsettled issues of textual determinancy and indeterminancy, we are prevented from proceeding forward with the kind of necessary degree of certainty that is required for consensus building.

It seems to me that we should be especially wary of those who centralize notions of "creativity" in technical communication. And it is difficult to imagine that we cannot agree over certain fundamental truisms: that technical communication is transactional, functional, work-related, informational, specialized, and denotational.

This is at the center of what we are discussing: as Bazerman says, "That debate over how to talk about one's subject continues in all disciplines today, and cannot be separated from the fundamental practices of those disciplines." [16, p.317] And, not only until, he notes, we understand the way symbols are used in the activity can we understand how to carry out the activity, but also--until we achieve general consensus--can we begin to carry out our activity. Especially useful, I think, is Bazerman's discussion in his text of 'how language realizes the work of science.'[see chapter 11, pp.291ff] He argues there that, while there are several reasons to distrust ideas of direct correspondence between scientific formulations and nature, there are means by which the links between language as social construct and as empirically based can be established. Bazerman analyzes what he sees as some of the causes of our "current inability to forge a convincing link."

> More specifically, what has been lacking is a unitary concept of signifying events simultaneously contexted within and realizing linguistic code, social relations, psychological cognition, and perception of the ambient world. [295]

A solution to the problem, whereby the code ("langue") is again tied to the meaning ("parole") and dialectically defines each other, produces an important reassessment of the entire process where balance rather than priority is the important issue.

> No doubt, regularities and structured elements appear in both language and society, but I know no reason to believe that either is prior to or privileged over the other. Until we have positive reasons for believing otherwise, we must assume society exists no more firmly, nor no less firmly than language (and other symbolic and physical means of coordinating activity). They are simultaneously realized in the social language act. Linguistic and sociological regularities--realized and institutionally structured in successive acts--might be best accounted for as parts of mutual realization. [p.301]

Karen Schriver, in her essay, "Theory Building in Rhetoric and Composition: The Role of Empirical Scholarship" comes to essentially the same conclusions, and makes some important points: that the best empirical scholarship is fully cognizant of the probabilistic and contextual nature of language and "realize the inherent limitations of any particular way of coming to know and share assumptions about the relativistic and problematic nature of certainty and knowing."[26, p.282] Also, that, as inductive inference represents the primary methods of investigation, its methodology is cumulative and historical, whereby it "extends, refines, organizes, or challenges previously held assumptions about reading and writing."[p.279] Inductive argument, unlike more purely deductive thinking, is less inclined to theory-building before data collection is sufficiently complete. To this extent, inductive research is a more deliberate, cautious, less

glamorous and incremental intellectual process than other methodologies. It would eschew, for example, the ambitious premises of Cultural Criticism --at least as it is articulated by Patricia Bizzell ("What I want to study is not local but global"); namely, that "interpretive activities are informed by ideological predilections" and that its ultimate concerns are political and moral in the sense that it brings one to conscious awareness of value systems and ideologies that are otherwise concealed or suppressed. [27, p.225-226] What Schriver writes about inductive empiricism in Composition Studies brings us full circle back to what Stevenson was arguing for Technical Communication earlier:

> Those who employ empirical approaches are concerned that their claims do not extend too far beyond the evidence collected. In fact, the empirical community is quite skeptical of those who overextend their claims, do not admit the limitations of their work, or ignore contradictory evidence in their own or in others' related work. A 'good read' in empirical work is one which carefully negotiates 'the play' between past and present arguments, between the intertextuality of the evidence. This rhetorical stance of guarded claims and qualified conclusions may leave some readers in English who are looking for the 'immediate teaching implications' or the 'move to grand theory' a bit frustrated or even bored. But such 'good reads' remind us to avoid sloganizing our findings and that conclusive pronouncements are not possible, or even desirable. [26, p.274]

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